An Interview with

DEAN BABCOCK

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Conducted by Arthur L. Norberg

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Charles Babbage Institute
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Abstract

After Babcock describes his early life, education and communications work in the Navy during World War II, the focus of the interview shifts to his work with Engineering Research Associates (ERA). Topics include: various ERA projects including Project Lime; the design of equipment at ERA; magnetic drum designs and capabilities; the work of Sidney Rubens and William Field on magnetic drums; and ERA's interaction with the Navy, especially during the Korean War.

DEAN BABCOCK INTERVIEW

DATE: September 12, 1986 INTERVIEWER: Arthur L. Norberg

LOCATION: Charles Babbage Institute (Minneapolis, MN)

NORBERG: Dean, you said that one of your first jobs on this campus was with Professor Otto Schmidt.

BABCOCK: Yes. When I started the university, I had one of the federal supported type jobs, the Youth Authority, whatever it was, and Otto Schmidt was in the physics department at that time. He was doing work on electromiographic potential, very early work. And I worked on building DC amplifiers for him, which was really quite a challenge in those days, since they had a way of drifting. And Otto was, of course, very unusual at that time to have two degrees, one in electrical engineering and one in biology. And so he was a biophysicist with an engineering background, or maybe, I guess his degrees were in physics and biology. And so he had a sort of an electronic physical approach to biological problems and electromiographic potential. That was a very fascinating job. And also we were working right next to the atom smasher - I actually did a little work on that, too, since I was worked on lots of different places in the physics department. The next big thing that happened was the Navy program. I went in the V12 program. And spent one year here on the campus in the Navy V12 program, which started in '43. And then I graduated in the V12 program along with Frank Klein, who's another one of the ERA pioneers. There were only, I think, six, five or six double E's that graduated in June of '44 in the Navy program. And then I went in to the Navy to go the Naval Academy and get my commission in Reserve School at the Naval Academy. And served on destroyers that was in the Atlantic.

NORBERG: Can we drop back just one minute and let me make an incidental comment before I ask you the next question. Otto Schmidt is still very active, I saw Otto yesterday. Can you tell me just a few things about your family and where you went to school initially and how you came to select the University of Minnesota?

BABCOCK: Okay. Well, I was born in Minneapolis and grew up on the northwest side. Went to Kennedy High School, the first graduating class from Dr. Kennedy High School in 1941. And as far as choosing the university there was essentially, there was no other place even in my consideration because I was aimed towards electrical engineering. I was a ham radio operator when I was twelve years old and there was just no place else as far as electrical engineering was concerned but to go to the university. So I started there. I started in the fall of '41 and graduated in June of '44. The Navy tried very hard to make a marine engineer out of me. Sent me to boiler school and a few things like that. Of course, with a name like Babcock, it goes along with Wilcox and Babcock.

NORBERG: Babcock and Wilcox, yes.

BABCOCK: And I tried very hard. And I did have to crawl through a few boilers and stuff before I got to be communications officer instead of an engineering officer.

NORBERG: What did it mean to be a communications officer on a destroyer?

BABCOCK: Well, the engineering officer, of course, tended to live in the engine room and in the boiler rooms and that sort of thing. The communications officer, on the other hand, tended to live in the radio shack. And for a former ham radio operator, especially one that could read Morse code, to be a communications officer was to be right where all the action was. I had a dual responsibility in that I had the normal engineering responsibilities of being responsible for everything electronic on the ship, which normally is an engineering responsibility. Even when I was communications officer, I still had all that responsibility; all the electronic technicians, I supervised all of them. Normally, that's an engineering officer's responsibility. But I kept sort of one foot in engineering and still I became the communications officer. I managed to accomplish that trick of going from engineering to communications by taking a communications officer course while I was waiting for my ship to come back to Norfolk. There was a course, a short course, it lasted three weeks or something like that, and I managed to get that under my belt so that then I could lobby for being converted to communications instead of engineering.

NORBERG: Did communications also involve any sort of electronic equipment like radar that might be on the destroyer?

BABCOCK: There was an officer on the ship who was a radar officer. And he was, actually later became a lawyer. So when he found there was somebody that understood electronics, he was very happy to have the radar technicians and so forth also, not exactly report to me, they still reported to him, but I spent a lot of time in the radar shack, in the CIC and so forth and all the technicians, I sort of had a rapport with all of them and it was kind of, you know, an unusual situation. I hadn't studied radar. Although when I graduated from the reserves, the Midshipman school at the Naval Academy, my first orders were to the radar school at Bowden. And when I originally got in the Navy that was my, I took the eddy test to become a radar officer, but then the timing happened to be such that they decided I should be in the V12 program and get my degree.

NORBERG: When did your ship arrive back in Norfolk?

BABCOCK: When was that? It was a month or two before the German surrender.

NORBERG: Okay, so about March or so.

BABCOCK: It was on the first trip to North Africa, with a convoy coming back from North Africa that the Germans surrendered. We picked up one of the German submarines that surrendered before Germany had surrendered, for instance, that was the timing of it. And then we made one more trip. We made two convoy trips. It was on the second trip actually that the Germans surrendered, that's right. So it must have been, I think the Germans surrendered in what...

NORBERG: May.

BABCOCK: May. So then that would have been early April, second trip. That would have taken two months. It used to take us two months to make a round trip so somewhere around February or something like that.

NORBERG: Still in early '45 at least.

BABCOCK: '44. Yes, '45.

NORBERG: How long did you stay in the Navy after Germany surrendered?

BABCOCK: Well, I stayed in until... I got out in April of '46. I arrived home from Japan; we went to the Pacific. We were converted to a mine sweeper, a destroyer mine sweeper, and we swept mines around Okinawa. While we were in Okinawa, the Japanese surrendered. And then we went up to Japan, up to Sasabo. Then we swept mines in the Sushima Straits between Korea and Japan. Then somewhere about April 5th or 6th, whatever it was, finally I got released and I came back to the States. The ship was still over there. I know I got home on Sunday night and spring quarter at the University started on Monday morning, classes started. So I arrived home on Sunday night and started classes on Monday morning.

NORBERG: To do what?

BABCOCK: Well, in the process of getting my degree in the Navy program, I had missed the spring quarter of my senior year and actually half of winter quarter, the Navy was on a semester system. So I came back and was able to complete those spring quarter courses even though I got my degree before I left, but I had missed that last quarter of radio engineering and a bunch of the other senior classes. I came back and took all of those, had those done.

NORBERG: I see. Who was teaching those classes at that time?

BABCOCK: Oh, golly, that's hard to remember. I know Sidney Larson was here. There was a young fellow that went with Newmann, when Newmann started his Lightning Transients Research Institute. I can't remember his name. He died of a heart attack just weeks before spring quarter was going to end. He had a bad heart. Let's see, Hartig was head of the department, and...

NORBERG: Do you remember the text used in radio engineering at the time?

BABCOCK: Oh, I think they were using Terman's book.

NORBERG: That's not something you remember because you've been in Palo Alto all these years I hope.

BABCOCK: Well, you know, Terman's book was like the Bible, you don't know where you first ran into it. I think I probably had a copy of that when I was a freshman or something like that and the handbook and so forth. But, let's see, Johnson had gone from being in the double-E department, I think he was in engineering, main engineering department as assistant dean or something like that. And I have a hard time remembering some of the professors' names. Remembering names is not my strong suit.

NORBERG: That's all right. So you came back, you finished those courses, was there any obligation to do that?

BABCOCK: No. The most important part of doing that was the fact that in the spring quarter there's a ritual which is that the electrical engineering students and certain other selected engineering students who are favored by electrical are allowed to congregate on the front steps of EE building to ogle at the girls as they go by at noon time. And that's what we were doing in May when an aeronautical engineer by the name of Bill Falk said to us, he said, "I went over to a company over in St. Paul, the Northwestern Aeronautical Company to see about a job, and they don't want aeronautical engineers. They want electronic engineers." And, of course, the ears popped up, mine especially, and I said, Where is this place? He said, "Over on Minnehaha Avenue." Where? So he gave me the address and that afternoon I was out in the old front building on Minnehaha Avenue and talking to Ken Busch about this company. And I talked to Ken Busch and Jack Boekhoff, Bob Patterson, and so they had me fill out an application blank and said when can you come to work?

NORBERG: This was in May of '46?

BABCOCK: May of '46, yes. So on the 24th of June, 1946, I started with ERA. I didn't have the slightest idea of what I was going to do except it was going to be electronics. Well, I had a lot of mechanical engineering courses. I took just about every mechanical engineering course I could, I took forging and welding and heat-treating of steel in the machine shop and kinematics and so on and so forth. And I think Larry Steinhart must have seen that in my, you know, on my transcripts and stuff, because the first project they put me on was the electro-mechanical accumulator. An electro-mechanical accumulator was to be an array of 50 by 50 cells of three digit electro-mechanical counters, 7500 of these little counters. And I was working with a young fellow by the name of Jim Ackerman and Jim Ackerman was an aeronautical engineer. And Jim Ackerman's dad was the head of the aero department here, Professor Ackerman --first head of the aero department actually. And so we were trying to build these counters, first the individual single cell and then we'd put three of them together. And the goal was to have them count at twenty counts a second. We had no idea. It was so many years later before I found out what the intent and purpose of that machine was. But it wasn't long that I decided the thing really was, the mechanical principles were bad. We were never going to get that thing to count at twenty counts per second.

NORBERG: What sort of mechanical principles were you using? Where had they come from?

BABCOCK: Well, one of the things which had been sort of key in the design - it had already been laid out, the general design - was a Western Electric relay coil which had ended up at Dayton, Ohio in the USNCML parts site, like 3,000 of them or something like that. They had been there for some other purpose and then they weren't used. And so the attempt was to use those for this accumulator. And it was not a good choice. What was needed was an attraction magnet, a solenoid or something instead of that. And later on, that coil was abandoned, probably ended up in a scrap heap someplace. But when Larry Steinhart came back I suggested that we go buy some of the various and sundry kinds of counters that were on the market, start from there rather than from where we had. But I discovered there were some communications projects going on at ERA and so I started working on getting out of this electro-mechanical thing and getting on these communications projects. And then somewhere along in there, Vern Vogel was hired and Vern took over that communications thing. And then I went to work on, sort of got out of the "computer business", if that was part of the computer business - you know, this project had started down at Dayton -

and I worked on, oh, I worked on the nine-channel, well, multiplex converter, demultiplexer, that we'd go up to 9 channels, anywhere from 4 channels up to 9 channels. It was a CXLF, I think it was, I think that was the nomenclature of it.

NORBERG: I see.

BABCOCK: Well, along about then, my clearances all got squared away. I was in the Naval Reserve at the Naval Air Station in an organized unit there, and then I transferred to the Naval reserve unit which was at ERA. Bob Patterson was the skipper. And it was a small group then, about 16 or 18, and almost all of the fellows that were in it had been in CSA during the war. So I was one of the first non-CSAers to get into that group. And, of course, then, as soon as I got into CSA then I started finding out what this was all about. You know, what all the work was all about. And went off to the various stations, Cheltenham, Winter Harbor and so forth, you know, on my reserve duty.

NORBERG: I'm not sure of the connection there, Dean. What does the reserve duty have to do with what was going on at ERA if anything?

BABCOCK: Well, the communications supplementary activity, you know, the CSAW, the reserve unit we had was a communications supplementary activity reserve unit. And so our, you know, home base was their 3801 Nebraska Avenue. See? So this little group was a group that was sort of part of the user while being part of the supplier. That came up just before I left, in about '50, the Korean War came around. But anyway, I worked on this demultiplexer, which was a mechanical demultiplexer - a big commutator that went around that you could change for the different systems and so on. And then I also worked on the...

NORBERG: Can I stop you there for just one moment? What do you know about the pre-history of the demultiplexer? Had anyone else from ERA worked on that earlier?

BABCOCK: Oh, yes. That was probably, I think that was the second in the succession of such demultiplexers. I don't recall right off hand who was, I can't think of who the project leader was on that when I first came on it.

NORBERG: I seem to recall that Bill Norris was working on something like that during the war when he was at Nebraska Avenue.

BABCOCK: Yes, probably.

NORBERG: Okay, I was wondering if you remember seeing any patent applications or anything of that kind that Norris' name would have been on.

BABCOCK: No. And we finished that machine and it went off the Cheltenham, and to other places. And then there was another one. I worked on I think it was the CXCW. It was a demodulator for frequency shift key signals. I worked on that. And I think Wally Wool. Wally Wool worked on those things; Dick Gill, of course, he worked on those things. Tom Robinson and they'd all come from ? . And then we finished up CXCW and that went off to the Navy. Then Art Hausman had started a project called Project Lime.

NORBERG: Lime? L-I-M-E?

BABCOCK: Yes. By that time, they were beginning to use words to cover these things as well as the Navy CX was, you know, equipment nomenclature, not a project nomenclature. And the project line was a pulse analyzer. And so Art in '49 left to go back to school - he went to Harvard or whatever it was to school, and then went to the Navy at 3801 Nebraska Avenue. About that time, they were starting the cadre for NSA. Art, of course, he went to NSA and became the head of RAID and all that sort of stuff. Art and Bill Boenning left at about the same time. I think they both had been working, they were both working on Lime. And then Alton Christianson became the project leader on Lime and I was his assistant. And we finished that equipment up, and I took that one to Washington myself and got it working again. Those things used to suffer very badly in the shipping from St. Paul to Washington. I can remember when the first computer went off in box cars and was essentially wrecked by the shipment by box car. That was before the days of hydro cushion and padded freight and all that sort of stuff.

I recall the story of the second computer that went off to Washington. By that time, they'd decided they should go in trucks. And the truck trailer it was in sat on the side of the road in Ohio someplace because the driver had trouble with his tractor and took his tractor off to get it fixed and then went to go visit his family in Ohio or something and left this $12 million worth of equipment sitting on the side of the highway, while people were going crazy wondering where it was. Things like that happened, of course, in those days. There wasn't the set-up for shipping electronic things; that came a little later on.

The Project Lime equipment went to Washington and I was the company engineer that got it working and so forth and so on. On Sunday I finished that and then on Monday morning I reported in uniform, now on active duty for two weeks, and I got the job of taking it to Cheltenham and then running all the tests on it and writing a report. And then sign the report and then got in my car, took my uniform off, and drove back to St. Paul and started on my next project.

NORBERG: This seems a little odd? Did no one ever question this sort of thing?

BABCOCK: No. There weren't too many cases where it happened, but it happened with a couple of those pieces of the CX (series), CXCW and so forth, because the fellows that knew about it, of course, were the guys that developed it and were there and they would go to Cheltenham on their active duty or to 3801 Nebraska Avenue when they were on active duty. And, of course, when the first computer arrived there at 3801 Nebraska Avenue, why then it wasn't long after that that several guys showed up there on their training duty. Of course, the computer was a much bigger deal, no one fellow would be in charge of its acceptance and stuff. Of course, acceptance testing was much less formal then than it is today. The specifications were, you know, five pages, or something like that, the whole specification for a piece of equipment was a couple of pages. There weren't all the other specs and books and stuff. We didn't have piles and piles of paper to work through. We all knew what it was we wanted and...

NORBERG: Well, how was that worked out? I was going to start back now with the development of specifications and moving through laboratory design and then into manufacturing to a finished product, but how would these specifications for the devices that you worked on in the CX series developed?

BABCOCK: Well, there was a group of fellows at 3801 Nebraska Avenue that would work up the specifications and so on and then discuss this with us, with our people out there. They would between them get a specification written up of the general types of things, you know, the band widths on the frequency shift converter, for instance. It was a real breakthrough, because it would demodulate the widest shift anybody could conceive of or the narrowest. It was very unique the way it was built. Most of the frequency shift converters at that time were designed for one specific shift and they wouldn't work for all these different kinds of shifts. Well, we had to handle shifts from like 85 hertz to I think the widest anybody was using was about two and a half kilohertz or something like that at that time. The standard, more or less standard thing that AT&T used was 850 cycles. But a lot of people used wider and narrower shifts. So we had to come up with a device that was adjustable. And it was a very, very clever...

NORBERG: How was that done? How would one move to a multiple range?

BABCOCK: Well, instead of using inductor-capacitor filters, which the classic Western, you know, Bell Labs, Western Electric type design, which had fixed filters in it, this used the Wien oscillator type filters, RC filters, active filters. It's one of the early cases of using active filters, active feedback, RC type filters. It was all classified so, you know, nothing showed up in the literature on it at all. And this was 1948, around 1948 when this was done. It was very clever. Hewlett and Packard were coming out with their model 200 audio oscillator at about that time. And of course, it was based on that same principle, and was a break through in audio oscillators. We had an audio oscillator that was a state of the art audio oscillator, wide-range audio oscillator, low distortion, that had been developed at Bell Labs that we used in labs. The Navy had had it. It was half the size of the desk here. That was a monster. It was huge. And the feedback as the line voltage would change and so on, either the amplitude would start building up on us or it would just fade away to nothing. It didn't have the stability. That was, of course, that was... To set, you know, the trend of technology at that time. That was the breakthrough that Hewlett Packard made on their first audio oscillator. And this filter, variable band width, variable spread filter discriminator, was right in there at that state of the technology. And if there have been problems in classification and so forth why the company could have gone off into making, you know, new types of filters and so forth that nobody else was making, active filters.

NORBERG: Okay, now the active filters had been developed for the Navy by Bell Labs? Do I understand that correctly?

BABCOCK: Well, I'm not sure that they were developed... The original filters of this type were developed at Bell Labs and then the fellows picked that up to do this work for the Navy. I don't know how much Bell Labs actually worked on doing that sort of thing for the Navy. They may have.

NORBERG: But that was not in the published literature, I take it.

BABCOCK: No, there was very little on that sort of thing.

NORBERG: Did you people know about the Hewlett Packard work before yours was finished?

BABCOCK: It was around that time we got the first of those oscillators. They came out just about somewhere there in the late '40s, early '50s, you know.

NORBERG: But you don't remember any connection between saying, well, Hewlett Packard has this, maybe we can do the same sort of thing with this design. Or look at what they've done, it's like ours.

BABCOCK: I don't remember any kind of connection like that, no.

NORBERG: How did you move from the design to some sort of prototype?

BABCOCK: Gee, I don't know. I guess that was just sort of the standard way radio engineering was done in those days. You built it up on a breadboard and got it working and then built another model on an aluminum, well, steel chassis in those days, and got it working that way. Then finally redesigned it again so it would be a maintainable, shippable type of unit. It went in its own little cabinet rack and so forth. I recall it was like 3 chasses high. Somebody designed a special power supply for it; it originally had been running on general purpose type power supplies. The design was cleaned up as far as the panel looking neat, you know, and symmetrical and so forth, and dials made, engraved type dials so you could set the band width without. Originally they were pencil marks on paper on the original design. And it went through, you know, the stages that were sort of traditional in those days of a breadboard where it was put together pretty much on a flat, you know, a flat sheet of metal or a piece of wood or whatever in the first step and then on to a chassis which the technicians made all the proper holes and soldered together. Then finally the third version had all the components...

TAPE 1/SIDE 2

BABCOCK: ...heard of, no one had heard of a printed circuit board in those days. The standard radio type of design, what we call point-to-point wiring where resistors and capacitors and things just went from tube socket pin to tube socket pin, pin-to-pin and so forth, but at that time, the Navy, the military services wanted all the components mounted on terminal strips and then wiring had to be cabled and go from the sockets and so forth. So you didn't have this rat's nest of components down underneath.

NORBERG: Did you people find this constraining in the equipment that you were trying to design?

BABCOCK: It took one more design cycle. It took an additional design cycle, because usually the fellows would... After the breadboard model, the first model after that usually would still have point-to-point wiring and it would go together and be much easier to modify. But of course, the services were much more concerned about the maintenance once it got in the field and vibration and shock and all those things. And that point-to-point wiring wouldn't take that vibration and shock. Once it got maintained without having terminals to attach things to, you had a reliability problem, because you take things off a tube socket terminal several times and things begin to break and it's hard to get good connections the second and third times and so forth. So it was a more formal way of building electronic hardware. It meant one more design cycle to do it, because you didn't sort of just naturally just start laying things out formally with everything on boards. You didn't know how many you were going to need in the beginning.

NORBERG: How much was your participation in each of these stages in going from the breadboard model to adaptation to a chassis and then into the Navy specified design?

BABCOCK: I tended to be involved in a lot of the early circuit design and getting the first breadboard models done and then sometimes in testing the units when they were finally assembled and actually running some tests, when we put them into a system, hooked them to the radio receivers and the antennas and so forth. The detailed design of the final item, which you might call the prototype item, I didn't get involved much in that. There were other fellows that did that sort of formal design. I was in the sort of exploratory design and then later on when it came to actually using the equipment and testing it in its use I'd get involved in that.

Now there's another phase, after working in that communications work for a while, I got involved with Sid Rubens in the magnetic work and that was one of the real fascinating things. And we went and set up our own new lab. There were a number of us in there. Dolan Toth was in there and I can't remember some of the other names, but there were several of us.

NORBERG: How early was this, Dean?

BABCOCK: This must have been about late '49 or '50 maybe.

NORBERG: So drums had already been constructed and so on, so you were beyond that stage.

BABCOCK: Yes. Well, there was an earlier project I worked on, fairly early, when the first drum, that big one, when that was built. I designed and built the read electronics for that.

NORBERG: Can you describe that project for me a little bit, your participation in it?

BABCOCK: Well, gee that was really early because I remember seeing Meader the very first time. That must have been probably in '47, I think. And now I can't remember who it was I was actually reporting to at that time, except I know that Boeckhoff was the head of all that stuff at that time. I can't remember who was actually... I was all by myself in one of those little office labs at that time. Bill Field was working on heads. So I worked with him quite a bit, because we had the drum and then one of the big problems were the heads because the signals that came out off that first drum varied in amplitude by about 10 db or so, because of eccentricities in the drum. And the first coating material on the drum was actually sheets of magnetic oxide film that were put on the drum like you'd put a stencil on a mimeograph machine. And it, of course, didn't lie perfectly flat and the drum had some eccentricity, and on a hot moist day the film would loosen up. It would expand and it would loosen up. So we were constantly trying to get the heads closer to the film without losing, and many times we lost the whole thing, of course, when the head would contact the film and it would start to tear. So I had the problem that the signals varied in amplitude by as much as 10 db. And then there was wow and flutter in the drum as it went around, so that the timing on the signals was very erratic. And they put the timing track on that drum statically, initially. There was a worm gear and a stepper that stepped the drum around and then it gave the timing head a new impulse each time. And so the timing tract itself was not very uniform and, of course, the read, someone else was doing read - the head driver for reading, I can't remember who that was. And, of course, that used the signal from the timing track to put the signals on the drum. And then I used the signal from the timing track when I took it off. But there was still all this timing variation. So we had to build buffers to take the signals off and buffer them to try to get them and square them up so that they would be in some kind of shape to go on the computer. And I remember I felt that progress meant going to a different kind of drum, myself. The run out problems in the drum - I mean it even had axial run out problems as well as radial run out problems. I had a friend who was in the industrial machinery business and so on and we were talking about this problem. He sold Dumore grinding spindles, and he said, "Gee, you know, these things are so good you can turn them at 30,000 rpm and you can't measure the run out on them. It's less than a ten thousandth of an inch or something." So I started lobbying at ERA for going to something like that to hold our drums so we could get the run out on the heads down to essentially zero, so that wouldn't be part of the problem. With this big drum, you know, the run off was one thing on a hot day and it was different on a cold day.

NORBERG: What exactly does run out mean?

BABCOCK: Oh, that's the eccentricity of the drum as it goes around. When you set the head spinning, you see it at one place on the drum and on another part of the drum it's different. And we were, oh as I recall, we were working with head spacings of about one and a half thousandths of an inch. Well, we were trying to get a density on the magnetic material at that time of 50 bits per inch. And in order to do that, you had to have a small gap in the head and then you had to have a very small gap between the head and the drum. And that was all set by the eccentricity of the drum itself and then, of course, the material that went on the surface. Later on, they started spraying the magnetic material on the head and then we'd spray it and grind it and spray it and grind it trying to get that eccentricity down. That was the primary thing that set the density you could get on the drum.

NORBERG: Okay, so the eccentricity would develop because of the way the tape was put on the drum not because the drum was fully machined.

BABCOCK: No, it would both.

NORBERG: Both?

BABCOCK: It was both. And it wasn't just that the drum was poorly machined. There's all the different factors, the bearings, the shaft it's on and so on. This drum was large so it was made up of several components...

NORBERG: Is this the 34 inch drum or the 16 inch?

BABCOCK: Yes, it was the big one.

NORBERG: 34?

BABCOCK: Yes, 34. I always called it the Maytag drum. The mechanical engineer worked at Maytag before and it kind of looked like a washing machine to me. But the problem was it was mostly aluminum and, of course, it expanded and contracted. And different pieces would expand and contract at different amounts, you know, depending on, how they were made and which way the material went and so on. So we were using a big drum to get a lot of surface and the big drum limited our density so big didn't mean any better. And it's like the problem, it's like the situation in going from an 8 inch floppy to 5.25 - to 3.5 - the 8" floppies had to have a low density because they were 8", because the material expanded and contracted and the eccentricities are related to the, you know, length of the radius. It was almost the exact parallel that floppies went through many years later. And so we went to the 16" drum, and then I think the next one was like 6" or something like that. Well, even with the 16" drum, which was built on one of these grinder spindles, the signals coming out of it were beautiful, just beautiful by comparison.

NORBERG: So they did accept your suggestion of taking the Dumore spindles?

BABCOCK: Well, I was one of the people that probably suggested something like that. And then, of course it went from there to the drums where motor and everything all began to be integrated and so forth. But, yes, that first, I think that first big drum was driven with a V-belt and when you wanted them to have uniform motion a V-belt's very bad, because a V-belt has joints and splices and takes a set every night when you shut it off and all that sort of stuff so the motion is full of wow and flutter. When you want nice smooth motion, you go to flat belt. Eventually we went to a flat belt drive and then finally we got rid of the belt completely and went to an integral design with motor and had that. That was after my days, I wasn't involved in that. Because after that and then going off to communication, then I came back into this magnetic computer business.

NORBERG: Had you been working with Rubens directly at that time or was there somebody else in between the two?

BABCOCK: Well, when... I don't remember when Sid came, but he wasn't there in the beginning. I think perhaps he came in '48, something like that.

NORBERG: Mid '46.

BABCOCK: Was it '46?

NORBERG: Around the same time you did.

BABCOCK: Oh, okay, but I think we made our connection through Bill Field, because when I was working on the read electronics I was working with Bill and Bill was doing the head design. And then any number of times after that I interacted with Bill. He was doing the pulse transformer design, which was, you know, absolutely key in vacuum tube computers. Vacuum tubes had all these characteristics which weren't really the best thing for computers. The impedance levels were too high, for instance. We had problems with propagating signals from one end of the computer to the other because you would like to send them on coax, but coax is 50 ohms and the tubes wanted to have like 3,000 ohms or something like that output impedance. And, of course, the input impedance was infinite. And so going from that high impedance down to a low impedance was quite a problem and pulse transformers were the only answer. And Bill Field's particular big contribution was the design of those things. And I used to interact with Bill some, because I always enjoyed magnetics.

You know, there was that limitation curve that the TI guy called it half micron apocalypse. Well, we almost had a vacuum tube apocalypse. Along in that period in maybe '48, somewhere in there, people were writing papers for national journals sort of proving that vacuum tube computers would never work because of the reliability problem. The mean time between failures was going to be 15 minutes or that sort of thing. And they were just about right in the early days. At one time I know we had tube engineers from GE, from Kenrad, and I think Tungsol, there were three tube companies that had engineers living with us trying to find out why the tubes failed, you know, why we had so much trouble with them. And there was a thing, that they later called sleeping sickness where certain tubes would be unpredictable in the computer circuits. And they finally found out that there was one of the rare earth oxides that were on the tube cathodes, which if the tubes sat there in a cut off bias position long enough, the tube was essentially paralyzed. And then when you'd send a pulse through, it wouldn't react. There weren't any electrons being emitted at that point. And so they had to change the tube cathodes and that eliminated that problem. And then of course they started designing computer tubes, which were ultra reliable, very reliable type tubes.

NORBERG: Did you participate in any of that discussion?

BABCOCK: Oh, just, you know, we sort of all did in a way. When problems came up, then the tube guys would come seek us out and say, hey what happened? And they'd try to reconstruct, you know, we'd do sort of postmortems whenever there was a failure trying to find out, trying to get to the bottom of what the problems were. We tried to build flip-flops with the first twin triode tube, which had originally been designed for the transponders, the IFF transponders during World War II. And it only had a single cathode. It was really two half tubes stuck in one, in one tube envelope. And that tube envelope only had seven pins, so it didn't have enough pins to have two cathodes. As I say, because of the size and so on and the technology, they made a sort of a half a plate and made a whole grid and cut half of it off and so you had only half a plate on one side and half a grid on one side and then a cathode in the center. And they found those tubes were unsuitable for computers, because when the grid jumped around it changed the cut off bias point. Well, you were designing for a given cut off bias in your flip-flops and so on, so this made them very unpredictable. And those tubes if you tapped them the flip-flop would go flip flop and they weren't supposed to do that. Actually we had thousands and thousands of those tubes that ended up being sold to the junkman. We went to the octal twin triode. Later a nine pin twin triode came out that we could use. So there were all the problems of using tubes and then, you know, at that point the naysayers, or whatever you call them, were doing mathematical computations and saying vacuum tube computers will never work because you need so many tubes and the reliability of the individual tubes is so bad...

Well, we would bring a computer up very gradually in the morning in steps. You couldn't afford to leave it on all night; too many things were apt to quit. Actually they would never turn the filament voltages off completely, just bring them down and then bring them up in about four or five steps in the morning and then finally put plate power on and hope it would start working. And towards '50 and so on it worked almost all the time. The picture really changed a lot from the early days. But it required that the tubes be improved. You couldn't just use, and new tubes designed, actually. First they started out with ruggedizing and making them better and then they started designing the new ones.

NORBERG: Some time ago I interrupted you when you started to say what you were doing in '49 when you left the communications and went back to working with computers again. And, as I recall, you said something about becoming involved with magnetics again.

BABCOCK: In the midst of this concern about vacuum tubes and their reliability, there was an attempt to build a computer which would not use vacuum tubes to build a magnetic computer. And so there were a number of building blocks that you needed. And, of course, one of them were flip-flops. In order to have flip-flops you have to active devices, amplifying devices. So we started out with the thinnest rolled magnetic material that you could get. That had originally been developed at the Naval Ordnance Lab during World War II and they called it Orthinol for NOL, on the end. The key to that material was the fact that the Germans had been building magnetic amplifiers during the war. It was one of the best kept secrets of the war. Their battleships had these magnetic amplifiers in their fire control sets and they never failed. The technicians even forgot all about how to fix them because they never failed. They were sort of all just transformers and diodes and things. There weren't any tubes or anything in them. So the idea was to try and take that technology up high enough in frequency so that you could build a computer. And our goal was to operate with microsecond pulses. In order to get higher and higher in frequency you have to have thinner and thinner material. So we go one-eighth mill thick orthinol to experiment with and to use. I remember I ran the first test under Sid's direction on those cores, wound the turns on them and so forth and ran the B-H curves on those materials. Then we started working to build magnetic amplifiers. And we built magnetic amplifiers that would amplify one microsecond pulses.

A little side light on that was one of the limits was diodes and we were using germanium diodes and they were point-contact germanium diodes at that time. As you go higher and higher in frequency, you want to drive the impedance level lower and lower because the distributed capacity and distributed inductance and so on is eating you up all the time and you wanted to have fewer and fewer turns of wire and lower and lower impedances. And you eventually get down to the point where the forward impedance of the diodes is too high. So we talked to the people that made diodes. Originally it was just Sylvania. And then GE got in the diode business. In talking to the GE guys, they said they had a new diode that they were working on and they sent us some experimental samples of this diode. The dc characteristics were wonderful because it was the first of the junction diodes. It was the beginning of this whole new era here of junctions. But of course they didn't tell us what was in those, we just got the things and they said here, run tests on them. And the dc tests were wonderful. And then we put them in our magnetic amplifier and then they don't work at all. And we went back to the dc test to see whether we had burned them out or what. So then I ran rf tests on the diodes, because we suspected the problem was that the junction capacity was high. And so we ran ac characteristics on them and sure enough we found that the capacity of the junction was high enough it just sort of swamped out the conduction part of the thing. And it's kind of curious, when I ran these tests and measured the capacity I found low and behold that the capacity varied when we varied the voltage. And I said, gee, that's kind of neat. Here's a capacitor that we can change by changing the voltage across it. And I wrote a little report and we sent it back to GE, and we said, "Say, you know, this might be handy as a capacitor that you can adjust, you see." And they sort of wrote back and apologized. They said, we're sorry, it didn't work. A few years later somebody at Hughes invented the varied cap. I showed how the capacity varied with voltage and all that sort of a thing. Well, then they did some more work and later diodes were more suitable to our magnetic amplifier work.

Then we got ferrite material. The Dutch had invented the ferrite material just before the war and had managed to keep it secret from the Germans all through World War II. Dr. Schnook in Holland was the one that had developed the ferrite material. So Sid wrote to him, or somebody wrote to him, and we got some samples back. The first samples were in the shape of cores for fly-by transformers for television sets because that was a big thing at that time that, you know, suddenly this whole new market for things. And the fly-by transformers had been a real problem with iron cores. Every television set made for the last forty years has had a ferrite core transformer in it. So they did hit the right market. But we needed little toroids so we could measure the fundamental magnetic characteristics of this stuff and start experimenting with it as a fundamental magnetic element. And Johnny Meelan ran the little model shop in our building and so I said to Johnny, can you drill a hole in this stuff, it's pretty hard. And he said I have carbide drills; I can drill a hole through anything. And he came back some hours later and threw all the broken pieces on my desk and he thought I'd pulled a joke on him, you know. He said, what kind of a joke are you pulling on me to try to drill a hole through that stuff, it's too brittle, you can't drill a hole in it. And so he was kind of mad at me. I spent a whole Saturday drilling one hole with a carbide drill and I wore the carbide drill out as fast as I wore a hole through that material because it turned out it was just as hard as the carbide. But we finally got a hole through it and then the model shop could cut a chunk of it out and make it into a cylinder and slice it up and make toroids. And then we ran the curves on it and, of course, that was the material we needed because that was very good at high frequency. It had all the right properties to go up as high in frequency as we had to go. It was with that material that we made the magnetic amplifiers that would amplify one microsecond pulses and we made the flip-flops.

Then we got the idea of storing information in those cores, and so we... The idea had been before with the rolled material, that in addition to this orthinol material, we'd gotten another one which had a square magnetic loop, orthinick I think it was called. And we tried storing data in those. But then we got to ferrite and we made some toroids, and we made, we actually made a four by four core plane. Nobody had invented the name core plane yet, but we actually stored 16 bits of information in this thing and read it out and shut it off and came back and turned it on and read it out again and so forth to prove that that would work. But the signals of course were very small coming out of the thing and that was considered a disadvantage. I don't think any patents were ever applied for on it because they thought, well, the signals are too small. Of course, later, the core planes became very important.

NORBERG: How did this compare with the work that was then going on at MIT?

BABCOCK: Well, we really didn't hear much about that MIT work at the time I was working on it. As far as we knew, we were doing original work. We hadn't seen anything on that.

NORBERG: You didn't see any reports of Whirlwind floating around ERA?

BABCOCK: I didn't. I didn't, no.

NORBERG: Anybody talk about them?

BABCOCK: Not about the cores, no. I remember, you know, people talked about Whirlwind, yes, but we were down at our laboratory level...

TAPE 2/SIDE 1

NORBERG: You mentioned the problems of trying to drill holes through this very difficult material. Once you had ascertained that the material was suitable for your purposes, did you then go back to the company and ask if they could manufacture them in the shape that you needed them?

BABCOCK: Oh yes. And for that matter, just a matter of a couple weeks after that first experiment of, you know, drilling the hole and getting the first toroids made, the shop got diamond tools and then we could fabricate whatever we wanted out of the diamond drills. And so then we could make things whatever size we wanted, because now you could machine this stuff reasonably with diamond tools. But we didn't have any diamond tools to start out with. We had diamond saws, but didn't have diamond drills. But then they did go back. But that's about the time I left. I left in June of '51, and the magnetic work was the last thing I did before I left.

NORBERG: Why did you come to leave? What was involved in the decision?

BABCOCK: Well, I couldn't find any mountains tall enough to ski on in Minnesota.

NORBERG: Simple as that?

BABCOCK: Well, see I didn't really consider... Another thing, I never really considered myself a computer guy and the communications group, it had its ups and downs. We were the only ones doing the necessary radio gear that you needed to get all the information that the organization needed, and so on, but they didn't need a lot of that whereas there was an awful lot of the computer work. And so I wanted to get back into communications work. And the clincher... Well, I had gotten into skiing, I was in charge of the ski patrol at a little ski area and I was running this little ski area and I was really into skiing as far as my avocation was concerned and one day I read an ad for the Collins Radio Company that they wanted engineers in California, not in Cedar Rapids, Iowa, but in California. And it only took one letter and that was it. I was off to California.

NORBERG: What sort of things was Collins doing at the time in California?

BABCOCK: Well, that was really interesting, because, of course, they were doing communications work. And when I got out there the first thing I sort of landed in the middle of a BHF transmitter development, and that was, you know, that was just right down my alley. But then I very soon got involved in things like the mechanical filter, which was an electro-mechanical magnetic thing. That was down my alley, too, because I had had the magnetic experience at ERA. I have six patents on that filter. And I worked on that and then I got to work on data systems, single side band, and all the very latest things in communications out there. When I joined Collins, it was called their Western Developmental Laboratories, and it was all development work. And so it was, you know, it was a fantastic opportunity for me to get into communications development. It was kind of funny, by the time I left there Collins was getting into the computer business. You couldn't keep these things apart.

NORBERG: A couple more questions on ERA before you go, I hope you don't mind. You've mentioned a few people that you've worked recently closely with. There are, of course, the names that pop out as the heroes of the organization and I'm wondering what your view of them was at the time. I'm thinking of people like Engstrom, Tompkins - Meader you mentioned but didn't say much about him, I assume you didn't really have much to do with him - and people like John Parker and Bill Norris.

BABCOCK: Well, of course, you have to realize when I started at ERA I was 22 years old. And I was, you know, the youngest kid on the block. And so they all sort of operated up at a (higher level). Now Tompkins, when I worked for Larry Steinhart, Tompkins office was the office next to Larry Steinhart's office and so I had interactions with him that were more extracurricular, whatever you want to call it. But I had a number of interactions with him. I enjoyed the association because he was a very philosophical guy, a great guy to talk to.

NORBERG: In what sense? Can you give a couple of examples to elaborate on that?

BABCOCK: Well, my favorite example was when he described, when we used to go to the cafeteria for coffee and so on, and he described two of his corollary duties at MIT when he was a professor at MIT. He was in charge of answering perpetual motion letters, and calendar letters. In order to handle the perpetual motion letters, he analyzed the perpetual motion machine business and identified all the fallacies and set up a taxonomy for perpetual motion ideas that people would send in. And then he had a paper he wrote on this and then he could answer the ads by just telling a person that had sent in a perpetual motion idea, your machine is in category A7 and then he would send them a copy of the paper and then they could read the paper and discover what fallacy was involved in their invention. So that took care of that.

But you can't answer calendar letters that way. People would send in their ideas for calendars to MIT, a big improvement on the Julian calendar, whatever. And so you can't answer them that way. You can't have a taxonomy of calendars, because calendars are pretty arbitrary. So he came up with a calendar that had 100 weeks, each had three days in it. And the other 65 or 66 days were vacations and holidays. But the work week was composed of three days that everybody likes, the three days were payday and Saturday and Sunday. And when people would write in with their calendar idea, he would say well thank you very much for your interesting, you know, your interesting letter about your calendar idea. It's wonderful, but I like mine better. And he would send his paper describing his idea about back. And he said he only got a couple of rebuttals that way and then could get on with what he wanted to do.

NORBERG: In fact, at any one time at ERA during the period you were there, there weren't more than I would guess maybe 150 professional people.

BABCOCK: About that. When I started I think I was the 17th engineer. For about two months, the paycheck came with Northwestern Aeronautical Corporation's name on it. We were all in the little building five to the front there by the fence. We were all there at that time. The only thing that was in the big building was the parts storage and the machine shop and so on and then, of course, all the administration was in the two-story building. All of the engineering was all in that little building out in front, number five. And it wasn't even full.

NORBERG: What I'm leading to is that suggests there should have been more interaction with people like Engstrom and Tompkins.

BABCOCK: Well, Tompkins was in building five, he was right next to Larry Steinhart. As you came in the door, Bob Patterson's office was immediately to the right, Steinhart's was right across, and Tompkins' was right next to Steinhart's at that time. But Engstrom wasn't there. Engstrom wasn't in St. Paul. Meader wasn't in St. Paul there in '46. They were just names to us. I saw Parker a couple of times, Parker was there, met him, you know, and talked to him a couple of times. The first time I met him, I didn't know who I was meeting, because the head popped in my little room and was looking for - I forget who - the first time, he said, is so and so here. And I said, no he's not here. And apparently he was going down the hall looking in every... Because everybody always had to be in their place and keep the door closed all the time because all the projects tended to be classified, you know. So we always kept the doors closed. So he was going down the hall. Well, then after a while he came back the second time, poked his head in and I said, no he's not here, he wasn't here last time or something like that. And then I later found out that oops, this was the president of the company.

NORBERG: Did you ever sit in on Arnie Cohen's courses that he was teaching on various aspects of logic design and computer design development?

BABCOCK: No, I was never involved in those.

NORBERG: Was there a similar sort of thing on the side of communications or magnetics or related areas?

BABCOCK: I don't remember anything that was formalized like a course, not that formal.

NORBERG: How about seminars within the company?

BABCOCK: Oh, there were, yes. There were, you know, what you would call seminars, where guys would get up and talk about, you know, the problem they were working on what they were doing and so on and get the rest of us informed, yes. It wasn't very formal. It was almost more like a project meeting sort of thing only more people would be involved, you know.

NORBERG: What sort of literature were you following at the time?

BABCOCK: Well, mostly *Proceedings* of the Institute Radio Engineers, and there were papers in the AIEE journal at that time. But I can't remember, you know, seeing a publication that was devoted to computers until maybe late '49, maybe '50 or somewhere in there before there tended to be any literature in the area. It was; papers that would show up in the other journals.

NORBERG: How about technical reports of other kinds that were not in what we would see as the standard journals?

BABCOCK: Well, we got some, Navy reports that came through. Some of those came from places like the Naval Ordnance Lab and the Naval Research Lab. I don't specifically recall seeing MIT reports at that time.

NORBERG: What about Princeton?

BABCOCK: That's kind of hard for me to remember when I first started seeing Princeton reports, but I know I saw them, you know, began to see them before I left ERA in '51. I saw some of those. Of course, when we got into magnetics, we were reading a lot of Bell Labs reports. Bell Labs and Western Electric reports. And some British reports.

NORBERG: How did they come across your desk?

BABCOCK: Oh, I think, I was... They came to Sid. All that sort of stuff seemed to come to Sid and then Sid would, you know, route them around to us. He was the focus for all the magnetic literature.

NORBERG: Did you attend professional meetings at the time?

BABCOCK: Yes. There weren't very many of them related to what we were doing, though. I went to Chicago that later became the... What was it? The electronic conference that was held in Chicago annually and I went to a couple of those while I was still at ERA. Some of the fellows were beginning to give papers there. We had a booth; that must have been about '50 at... What the heck was that thing called that was in Chicago?

NORBERG: It was the National Electronics Conference.

BABCOCK: Yes, the National Electronics Conference.

NORBERG: There was one in '47, another one in '49 that I remember. I don't remember one in '48.

BABCOCK: Well, probably it might have been the one in '49 where we had a booth and we displayed the drum and Bill Field's pulse transformer. You know, the things that we could display that were things that the company wanted to sell and that were not involved in any of the classified stuff. We had a booth there and I remember I spent some time in the booth. Bill Field was there, and Jim Miles I think was there and a few others like that.

NORBERG: Do you remember any interaction with the customer? The Navy, principally?

BABCOCK: Well, yes. You're not speaking specifically of the USNCML?

NORBERG: Well, I was going to get to that, if you didn't mention it.

BABCOCK: Yes, well we had a lot of interaction with those fellows. For that matter, in the early days, the Navy people were our assembly workers. We had Waves, we had about twenty, I think when I came to work there was about 24-25 Waves. And we had an Ensign leader, Lieutenant Klein, who was in charge of an assembly area in which the Waves put things together for us. One of my more dramatic experiences, for instance, was the time that one of my designs for a cable came back in the hands of one of the Waves who had red hair and flashing green eyes and who's name was Brownie - and when she found out I was the designer of this thing, I got a whack in the head. She'd misinterpreted something in my drawing and put it together wrong and it had failed inspection and she was unhappy about that. So I got a whack in the head. Of course, the Waves were disappearing like one every month or something like that, or two a month, as their time ran out, and of course, the Navy had no intention of replacing those people. But we had that interaction.

And then of course, we had the security interaction. The Navy supervised the security. They used to run around and see if they could get in our rooms and get into our files and get through our windows. They even had a particularly - what would you say? - ambitious, or whatever you want to call it, First Class Petty Officer who had a clothes pin rigged on a pole with a string and would reach in through the screens on the windows when we were out to coffee or something to see if he could snatch a drawing off the desk and pull it through the window. And he succeeded a couple of times. Their job was to make sure that we were living up to all the security requirements and so on. But then the, you know, single most noteworthy interaction with the Navy was with Ed Svendsen, when he was there. He used to really come out and, you know, and get close to the projects and so on. And so I got to know Ed better than any of the others. He was the most technical of the officers that was there. I had run into him, he did graduate work at the university here when he was in the Navy and I first met him there. So then when he was there at

USNCML, I got to know him.

NORBERG: How did he interact with people in ERA?

BABCOCK: Well, he would actually come out to the labs and, you know, talk to us on the job of what we were doing, how things were going. He interacted on a technical level with us about the ideas and the usage things were going to be put to, you know, whether we were on the mark or not and so on. It was like having a technical monitor, a resident technical monitor sort of. And he had a pretty general interest on a lot of different projects so he showed up in an awful lot of different places. It was kind of a unique situation, because at Collins I was the fellow that wrote the proposals for NTDS before it was even called NTDS on the links. It was originally called something like the Three Ships Data System or something. Well, actually, no the first proposal was we were going to send radar data back from a blimp, the radar in a blimp. That didn't pan out. And then there was a Lockheed airplane we were going to send radar data back from on a data link, and then finally the Grumman, which finally did materialize. That was later called ATDS. Then we started doing proposals on NTDS data links and so forth. And then I ran right back into Ed again. He was Mr. NTDS. And that was the first, you know, and that was very appropriate, because that was the first big system that wed the computer and the communications systems.

NORBERG: In the Navy.

BABCOCK: Yes.

NORBERG: Because wouldn't SAGE classify as a competitor for that title at the same time.

BABCOCK: I think what I'm thinking of is a system out in the forces, you know, in this case on board ship and so forth. And so it sort of indirectly to the takeover of the Collins Radio Company. Rather interesting. Because when we started that work and we got to a certain point, we were told to get together with IBM, because you know, the things going to be a marriage of communications and the computer. Well, Arthur Collins' idea of that arrangement was that IBM would be the subcontractor to Collins. That didn't fit very well in Ednicott, New York. And then it turned out it wasn't IBM, it was Sperry. And then it suddenly became Sperry. And then there was a little jockeying as to whether Sperry or Collins was going to be prime contractor. It turned out that Collins was the contractor on the links and Sperry on the computer and so forth. But that put the bug in Art Collins' bonnet that Collins had to develop a computer for communications. And it wasn't too many years later that they actually started the development of computers in Burbank. I left about the time they started developing the computer. The computer just captured Art's imagination. And it got to the point where he was trying to develop a computer and couldn't, and sort of, you know, he got into the technology computer development leap-frog type of operation. And the next thing you know there was so much money tied up in that the company was having financial difficulty. And then Rockwell came in. So in a way that NTDS experience and the marriage of the computer and communications was sort of the unraveling of the old traditional communications thing in Collins and he lost control. That's the way those things go. But...

NORBERG: Good, this has been very informative. I appreciate it very much.

BABCOCK: Yes, I'm not sure you were in the room. Were you when I mentioned the role of the reserve group there and...

NORBERG: This (machine) didn't hear it in the room.

BABCOCK: Well, when I asked how many people in the room were in that reserve group and the largest it was when I was there, up to '51, was only about maybe 27 or 28 or so on. And I was amazed at how many hands went up, how many people were actually in that reserve group.

NORBERG: It seemed like 25 went up in that room, at least.

BABCOCK: At least. And so I asked Patterson about it. Bob Patterson was the skipper, and he said it got up close to 50. There were around 50 in the group at its peak. So, you know, when you think of the ones that went in and out, there must have been somewhere on the order of 80 or so people that were in that reserve group at some time. And it had a unique role in that it was a kind of a hole in the wall between CSA activities and the company. It was a way for the intent and the special needs to flow from that part of the Navy into the company in a very special way, because the people actually went out to the stations and saw the things happening and went to headquarters and knew what was going on there. And not everybody in the company could know all that, obviously.

NORBERG: Would you say that among these 80 people or so that might have been part of this unit over the years, that they were primarily professional people, maybe all of them?

BABCOCK: Not all, no. Because we had... I mean it was a top heavy reserve unit as far as officers were concerned. When I was in there there was like 18 officers and 6 enlisted men. There were guys like Robby, well, the only new guys that came into it were officers. The enlisted men came from World War II. They came from the old CSA organization during World War II. Dick Gill was a warrant officer. He had been an enlisted man and was a warrant officer by the time the war was over. Don Yakaboni was a chief petty officer. And Robby was a first class petty officer I think. I can't remember any more of the other names. But there weren't more than about 6 enlisted men and then 18 officers. And the new guys coming in were officer types. And then of course...

NORBERG: So this really does emphasize the point you just made about being a special window onto the customer needs.

BABCOCK: Yes. We tended to be the only people that went out to the field sites, went to Winter Harbor, went to Cheltenham and those places where the data was being gathered and such. And also at 3801 Nebraska Avenue when we were there on reserve duty, we went in and out through all those doors, where a general company guy would only go if it was sanitized. So we saw what was happening. You know, we could only actually discuss it when we were in our own little group. So it was a kind of a unique little ganglion that connected these two things. It died with the Korean War.

When the Korean War started, I had training duty scheduled at Washington. There were a lot of my friends that bid me goodbye. They said we'll see you in years, you know, see you at the end of the war. They never expected me to come back. And so I went to 3801 Nebraska Avenue and the very first day I went to find the assignments officer, and I said, "You know, I want to know. Am I going to go on active duty or am I going to stay in St. Paul?" And he reached in the bottom left drawer of his desk and he pulled out and he says here, this is the stack of orders on the whole unit, except for one. He said there's orders in here for everybody here in the reserve unit, except Don Yakaboni. Somebody had decided they needed Don already and when they found out there were orders cut for him, they pulled them out of the stack got him signed and sent them to him. So Don was the only guy in the organization that got called to active duty in the Korean War. But then when they got a deferment, a presidential deferment or whatever it was for the whole group, for the whole unit, that was the death knell for the unit. It got disestablished.

NORBERG: I see. And who went about getting this deferment?

BABCOCK: Well, I think it was probably NSA.

NORBERG: I see.

BABCOCK: They said the last thing we want to do is to pull those people out of ERA. They're all building the stuff that we want, the hardware we want. It doesn't do any good to pull them into active duty and send them out to the stations and cripple that operation that's giving us the equipment that we need.

NORBERG: Which would be consistent with the arguments made in '46 for founding ERA in the first place.

BABCOCK: Right. And so other than Don, who went off to set up new radio stations in the places where they needed new radio stations, the rest of us stayed there and kept working on equipment that they needed. But the policy on the reserve, there were a few other reserve groups where people didn't get called up and they said we can't have organized reserve units which can't be called up. That's sort of a self-defeating policy. And so during the Korean War there shortly after I left ERA, it was disestablished. So it had a unique role, but it couldn't go on forever that way.

NORBERG: By that time, ERA was beginning to have its cash flow problems anyway, and so this wouldn't have made any difference one way or the other, I think.

BABCOCK: Yes. And also, what you might call the liaison between the Navy and ERA and so on was becoming more formalized. This informal liaison through this Naval Reserve Unit was in a way becoming either of less importance or more awkward to maintain, shall we say.

NORBERG: Why?

BABCOCK: A more formal liaison was being established.

NORBERG: Why do you think there was a more formal liaison being established?

BABCOCK: Well, just the fact that that was the whole business of the way specifications were happening and the way stuff was being developed and the newer regulations on the interaction between industry and the government and so on. That was all becoming much more formal.

NORBERG: Okay, so this was an independent interaction that had nothing to do with ERA.

BABCOCK: Yes. And you know as more and more of the people... Because in '46 a number of the key people were still on active duty. And one by one they were going to civilian life and so forth and now all the sort of informal, which really was vertical from the top to the bottom, we had technicians who had this informal liaison and we had admirals. And then slowly it all moved across, and it was essentially all the way across, and then the Korean War came along and it couldn't continue that way. I mean you couldn't be sort of both in the Navy and in the industry at the same time, just because of the mobilization policies more than anything else. And so that fixed that and it just sort of stopped that way. And at the same time, there's an incidental thing in all the deal. I don't know if it was around that time it became DOD.

NORBERG: '47 it became DOD.

BABCOCK: Yes. And all those DOD type things were coming into being.

NORBERG: Well, very good. Thank you very much.

END OF INTERVIEW